

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	The impact of covid-19 restrictions on behavioural and psychological symptoms in home-dwelling people with dementia: a prospective cohort study (PAN.DEM)
AUTHORS	Gedde, Marie; Husebo, Bettina; Vahia, Ipsit; Mannseth, Janne; Vislapuu, Maarja; Naik, Mala; Berge, Line

VERSION 1 – REVIEW

REVIEWER	van der Wardt, Veronika Philipps-Universität Marburg, Department of Primary Care
REVIEW RETURNED	21-Apr-2021

GENERAL COMMENTS	<p>Many thanks for this interesting analysis, there are a few issues that should be addressed:</p> <p>Major:</p> <p>The statement that 26% of people who did of covid had dementia is very bold not not substantiated by the referenced website (which includes a large number of non-peer reviewed reports). It is not clear where this information comes from. Please clarify or amend.</p> <p>In the introduction, the studies investigating the effect of covid on BPSD require more detail to provide the context of the findings (setting, degree of dementia, carers if applicable, etc)</p> <p>In the discussion, the effect of the carer's own psychosocial factors on their perception of the person with dementia's BPSD/depression/ADL should be discussed in more detail. At the moment this is one sentence in the limitation section. The relationship between carer's psychosocial factors and perception of BPSD is complex and the discussion should aim to explore that in the light of the findings (see Feast A, Orrell M, Russell I, Charlesworth G, Moniz-Cook E. The contribution of caregiver psychosocial factors to distress associated with behavioural and psychological symptoms in dementia. International journal of geriatric psychiatry. 2017 Jan;32(1):76-85.)</p> <p>It is not clear on what the conclusion regarding the importance of clear communication with carers and health services is based on (page 13, lines 54-57). How does this relate to the findings?</p> <p>Based on the results, the recommendation of anxiolytics and hypnotics/sedatives should be discussed.</p> <p>Minor:</p> <p>In 'Strength and Limitations of the Study' (page3/4 - depending on which page number is chosen), the statement 'the covid restrictions left some of the raters...' needs clarification.</p>
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	Figure 2 should be changed into a histogram
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REVIEWER	Cipriani, Gabriele Versilia Hospital
REVIEW RETURNED	25-Apr-2021

GENERAL COMMENTS	The article is well written and worthy of publication
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REVIEWER	Hunt , Lauren UCSF
REVIEW RETURNED	06-Aug-2021

GENERAL COMMENTS	<p>This observational prospective cohort study leveraged an existing parent study to assess the impacts of SIP lockdowns on behavioral and psychological symptoms in people with dementia. They assessed BPSD symptoms just prior to lockdown and 6-9 weeks after lockdown occurred in 104 dyads. Assessing impacts of covid-19 on PWD and BPSD is a very important area of study. We must consider trade-offs of different pandemic management approaches and how we can better support caregivers through what looks like will be continued on and off SIP orders over the next several years. However, I have significant concerns about the results and statistical analysis of this study.</p> <p>Major concerns:</p> <ul style="list-style-type: none"> • I'm particularly concerned about the wide distribution and skewed nature of the BPSD measures with SD larger than the mean. In this case, I would consider reporting medians and interquartile ranges or categories instead. • I'm also concerned about the use of logistic regression in this case. Logistic regression is intended for binary outcomes, but the authors don't say how they dichotomized their measures. These unclear outcomes, combined with small sample size and incredibly wide confidence intervals, makes these results essentially meaningless. • I would consider taking out the logistic regression results about predictors of BPSD and just reporting the pre and post values. I don't think the predictors of BPSD adds a whole lot to this article.
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REVIEWER	Kinosian, Bruce University of Pennsylvania
REVIEW RETURNED	14-Aug-2021

GENERAL COMMENTS	<p>Useful prospective study of the impact of covid restrictions on BPSD; doesn't specify the restrictions, nor how selective the cohort is. The impacts (in terms of increase or decrease in carer contact) are hard to interpret without a baseline level (the relationship between living alone and having a carer contact them and who carers are is not well described, nor the relationship of living situation w the carer). In some sense, with 56% living with carer, it's curious only 22% increased contact during the lockdown; unless that implies a very high level of carer burden in terms of contact pre-pandemic.</p> <p>Nested prospective cohort study within RCT ? intervention effects Selection effects—most from non-contact; although still 1/3 declined after invited. Need to know how study sample differed from study population.</p>
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	<p>flow chart has institutionalization numbers--- were rates above or below expected- how much selection from censoring?</p> <p>Cognitive criteria not coextensive--- MMSE 15 not equivalent to FAST 7</p> <p>Caregiver contact covariate; ? carer living status not included?</p> <p>Half of eligible elected to participate? Reasons? Have characteristics of volunteer dyads, but not clear how much selection without a set of comparative characteristics on dyads from the parent study who did not participate.</p> <p>Need to specify the specific covid restrictions in methods—e.g., 28% had reduced contact, but 44% lived alone; what does that say about either lockdown adherence, rules, or about the level of pre-pandemic contact? How is psms scaled? OR suggests protective effect w higher scores- - is that independence or dependence?</p> <p>Impact of prn psychotropics on depression but not psychosis interesting; would think they'd be a flag for worsening sx. Increase or decrease in contact hard to interpret in absence of level of contact pre? Randomization wasn't shown to affect pandemic response, but not sure what the power of the contrast is P12 I50--- parent study recruited to be representative; but strong selection into this substudy?</p> <p>P 13, I 35--- dementia stadium? Assertion that pandemic responses were similar across countries unclear—e.g., Sweden vs Norway.... Would be helpful to know what restrictions were.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Dr. Veronika van der Wardt, Philipps-Universität Marburg

Comments to the Author: Many thanks for this interesting analysis, there are a few issues that should be addressed:

Major: The statement that 26% of people who died of covid had dementia is very bold not substantiated by the referenced website (which includes a large number of non-peer reviewed reports). It is not clear where this information comes from. Please clarify or amend.

Response: Thank you for this important comment. The citation is now corrected, and the URL directs directly to the report stating that 26% of people who died of covid-19 by August 2020 had dementia.

In the introduction, the studies investigating the effect of covid on BPSD require more detail to provide the context of the findings (setting, degree of dementia, carers if applicable, etc)

Response: We thank you for your invitation to elaborate on the context of previous studies related to covid-19 and BPSD. The introduction is re-formulated according to your suggestions (lines 76-84).

In the discussion, the effect of the carer's own psychosocial factors on their perception of the person with dementia's BPSD/depression/ADL should be discussed in more detail. At the moment this is one sentence in the limitation section. The relationship between carer's

psychosocial factors and perception of BPSD is complex and the discussion should aim to explore that in the light of the findings (see Feast A, Orrell M, Russell I, Charlesworth G, Moniz-Cook E). The contribution of caregiver psychosocial factors to distress associated with behavioural and psychological symptoms in dementia. International journal of geriatric psychiatry. 2017 Jan;32(1):76-85.)

Response: We appreciate that you invite us to discuss the important issue on the carer's psychosocial factors which is now elaborated on considering the valuable reference you provided us with as well as covid-19 specific literature (lines 249-256).

It is not clear on what the conclusion regarding the importance of clear communication with carers and health services is based on (page 13, lines 54-57). How does this relate to the findings?

Response: Thank you for noticing this statement. We agree that the conclusion is not sufficiently underpinned by our findings, and we have therefore decided to remove this statement from the manuscript.

Based on the results, the recommendation of anxiolytics and hypnotics/sedatives should be discussed.

Response: Thank you. We recognize that this aspect was not discussed appropriately, and the findings are now discussed in relation to the national treatment guidelines (lines 286-289).

Minor:

In 'Strength and Limitations of the Study' (page 3/4 - depending on which page number is chosen), the statement 'the covid restrictions left some of the raters...' needs clarification.

Response: We are grateful that you identified this unclear statement. We have now clarified the statement in question according to your request, which now reads: "The covid-19 restrictions left some carers with less basis of observation, as 28% reported reduced contact with the person with dementia" (lines 57-58).

Figure 2 should be changed into a histogram

Response: Thank you so much for this advice. We hope that the new figure is appropriate to provide the reader with necessary information (figure 2).

Reviewer: 2

Dr. Gabriele Cipriani, Versilia Hospital

Comments to the Author:

The article is well written and worthy of publication.

Response: We thank the reviewer for the kind words and the recommendation for publication.

Reviewer: 3

Dr. Lauren Hunt, UCSF

Comments to the Author: This observational prospective cohort study leveraged an existing parent study to assess the impacts of SIP lockdowns on behavioral and psychological symptoms in people with dementia. They assessed BPSD symptoms just prior to lockdown and 6-9 weeks after lockdown occurred in 104 dyads. Assessing impacts of covid-19 on PWD and BPSD is a very important area of study. We must consider trade-offs of different pandemic management approaches and how we can better support caregivers through what looks like will be continued on and off SIP orders over the next several years. However, I have significant concerns about the results and statistical analysis of this study.

Major concerns: I'm particularly concerned about the wide distribution and skewed nature of the BPSD measures with SD larger than the mean. In this case, I would consider reporting medians and interquartile ranges or categories instead.

Response: We have now considered the distribution of all measurements based on this valuable feedback. We now report nonparametric data by n (%) or median and interquartile range; the latter applies to the BPSD measures. Consequently, we assessed change in BPSD by Wilcoxon matched-pairs signed-rank test. We detected significantly higher medians for NPI-12 total score after the restrictions occurred than before (20 [7, 32.5]

vs 16 [4.5, 29], $P = 0.03$), while change in other BPSD remained consistent with what we initially reported (table 3).

I'm also concerned about the use of logistic regression in this case. Logistic regression is intended for binary outcomes, but the authors don't say how they dichotomized their measures. These unclear outcomes, combined with small sample size and incredibly wide confidence intervals, makes these results essentially meaningless.

Response: We agree that the core information of our study is change in BPSD. We have now rephrased the Statistics paragraph to include dichotomization, stating: "Next, we dichotomized those NPI-12 and CSDD sum scores that changed into worsening/not worsening and utilized multiple logistic regression analysis to explore factors associated" (lines 157-158), and added to the table legends, accordingly. The limited sample size and wide confidential intervals of estimated odds ratios are now addressed under study weaknesses (lines 248 and 260-262). We have changed the term 'explanatory variables' to 'covariates' respecting your concern (lines 159 and 163). Also, to reduce the relative importance of these findings in question to the pre- and pandemic values, we decided on presenting the models as supplementary material (supplementary table A).

I would consider taking out the logistic regression results about predictors of BPSD and just reporting the pre and post values. I don't think the predictors of BPSD adds a whole lot to this article.

Response: We appreciate that you find the descriptive pre- and pandemic values an important contribution to our field of research. We now provide the logistic regression results as supplementary material (supplementary table A) to reduce its relative importance.

Reviewer: 4

Dr. Bruce Kinoshian, University of Pennsylvania

Comments to the Author: Useful prospective study of the impact of covid restrictions on BPSD; doesn't specify the restrictions, nor how selective the cohort is. The impacts (in terms of increase or decrease in carer contact) are hard to interpret without a baseline level (the relationship between living alone and having a carer contact them and who carers are is not well described, nor the relationship of living situation w the carer). In some sense, with 56% living with carer, it's curious only 22% increased contact during the lockdown; unless that implies a very high level of carer burden in terms of contact pre-pandemic.

Nested prospective cohort study within RCT

? intervention effects

Response: This cohort study was nested within the stepped wedge randomized parent trial as the covid-19 restrictions halted original trial protocol. Thus, we understand your concern for remaining intervention effects under the pandemic scenario. Our analyses did not reveal such intervention effects (lines 223-226, supplementary table B). We have now added to the legend of supplementary table B that just 21(20%) dyads out of 104 had received the intervention as the restrictions took effect. The skewed ratio was caused by the stepped-wedge design of the parent trial.

Selection effects—most from non-contact; although still 1/3 declined after invited.

Need to know how study sample differed from study population.

Response: Thank you for this crucial reflection. 21(14%) of the invited carers to PAN.DEM were not included of which 3 declined participation. We have adjusted the flow chart in figure 1, accordingly. We compared our PAN.DEM study sample to those not included yet still in parent trial to investigate whether bias was introduced. Comparing the samples on 22 variables derived at the pre-pandemic assessment, we find the samples to differ on five comparisons (lines 225-228, supplementary table C). We therefore suggest that our cohort is unlikely to suffer from selection bias (lines 243-245).

flow chart has institutionalization numbers--- were rates above or below expected- how much selection from censoring?

Response: At the six-month pre-pandemic assessment, we experienced 15% loss to follow-up dominated by institutionalization. Dyads were recruited to allow for 20% loss to follow-up, and

as such, attrition rates are within what we estimated. Notably, we expect increasing attrition rates with disease progression conducting the study. The total number of dyads lost to follow up over the whole two-year period are not yet available as the parent trial is still ongoing. This is mentioned in the legend of figure 1 (line 362), explaining that parent trial attrition was within assumptions of loss to follow-up.

Cognitive criteria not coextensive--- MMSE 15 not equivalent to FAST 7

Response: Thank you. We agree that the inclusion criteria of cognitive and functional impairment are not coextensive. These overlapping criteria (MMSE 15-26 and FAST 3-7) were chosen for the parent trial to secure recruitment of persons with dementia and their informal carers living at home.

Caregiver contact covariate; ? carer living status not included?

Response: Thank you for drawing attention to the logistic regression model selection. In the research group, we discussed relevant covariates before and during the process of fitting models based on our observations from the PAN.DEM telephone interviews, geriatric/gerontopsychiatric outpatient clinics/wards, and society at large. Carer living status intersected with other demographic covariates and the caregiver contact covariate and was omitted because the other covariates were considered more important. After fitting our models, the final ones were checked for multicollinearity, robustness, and goodness-of-fit. Yet, due to the uncertainty of estimates, the results should be interpreted with caution.

Half of eligible elected to participate? Reasons?

Response: The first weeks of the covid-19 pandemic were used to obtain ethical approval for PAN.DEM. We then consecutively invited as many dyads as possible from a list with no background information on the dyads that could have influenced sample selection. Ideally, our study would have prospered on a larger sample, but this was not possible as the pandemic situation developed. We now provide the reader with more detailed description in the method section (lines 110-112) and a sensitivity analysis comparing the PAN.DEM cohort to those not included in the parent trial (lines 243-245, supplementary table C).

Have characteristics of volunteer dyads, but not clear how much selection without a set of comparative characteristics on dyads from the parent study who did not participate.

Response: Thank you for drawing attention to the possible bias from consecutive sampling. The supplementary table C provides comparative characteristics on dyads from the parent trial not included in our study sample.

Need to specify the specific covid restrictions in methods—e.g., 28% had reduced contact, but 44% lived alone; what does that say about either lockdown adherence, rules, or about the level of pre-pandemic contact?

Response: Again, thank you. Analyses investigating the relationship between change in contact and residency showed that most carers not living together with the person with dementia reported reduced contact yet some violated the restrictions attending their obligations as carers. Respecting your request, lockdown adherence is addressed on lines 258-262. The restrictions are now specified in the methods section (lines 104-107) together with some clinical consequences listed in the introduction (lines 66-68).

How is psms scaled? OR suggests protective effect w higher scores-- is that independence or dependence?

Response: Thank you. OR indicates an inverse association of dependence in activities of daily living by PSMS and worsening psychotic symptoms. This is now clarified in the result section (line 215).

Impact of prn psychotropics on depression but not psychosis interesting; would think they'd be a flag for worsening sx.

Response: Many thanks for inviting us to discuss the use of psychotropic drugs and worsening psychosis and depression. We now reflect on these issues on lines 277-280 and 286-289.

Increase or decrease in contact hard to interpret in absence of level of contact pre?

Response: We agree in your concern. We know that all dyads had weekly face-to-face contact (parent trial inclusion criteria). However, residency indicates pre-pandemic level of contact indirectly (table 1). As we do not have more specific data on pre-pandemic contact level, the carers estimated whether the contact with the person with dementia changed as restrictions were imposed. This is now clarified in the table 2 legend.

Randomization wasn't shown to affect pandemic response, but not sure what the power of the contrast is

Response: Thank you! No participants received the intervention as the PAN.DEM assessment was conducted because the intervention was incompatible with the pandemic. We found no remaining intervention effects comparing those receiving the intervention before the pandemic to those who was scheduled to receive the intervention as or after the pandemic hit (lines 225-228, supplementary table B).

P12 I50--- parent study recruited to be representative; but strong selection into this substudy?

Response: Supplementary table C now provides the pre-pandemic characteristics of our PAN.DEM study sample and of dyads not included in our study sample. Lines 225-228 declare that the characteristics were largely agreeing, indicating minimal bias by selection (lines 243-245).

P 13, I 35--- dementia stadium?

Response: This is now corrected to dementia severity (line 243).

Assertion that pandemic responses were similar across countries unclear—e.g., Sweden vs Norway.... Would be helpful to know what restrictions were.

Response: This is an important aspect. The Norwegian Government had six strategies for gaining control over the spread of the virus: 1) hygiene measures, 2) isolation of infected persons and 3) quarantine of their close contacts, 4) restrictions on movements, 5) reduced social contact within the population, and 6) comprehensive protective measures for high-risk groups including visitation-ban for patients receiving 24-hour care. Some of the clinical consequences for home-dwelling persons with dementia are now listed in the introduction (lines 66-68) together with the restrictions (lines 104-107).

VERSION 2 – REVIEW

REVIEWER	van der Wardt, Veronika Philipps-Universität Marburg, Department of Primary Care
REVIEW RETURNED	01-Oct-2021

GENERAL COMMENTS	<p>This is a well written paper with an interesting analysis of predictors of change in BPSD in people with dementia during the covid-19 restrictions in Norway. There are a few issues that should be addressed:</p> <p>Abstract: please add mean time between assessments (and SD). There is a type on page 3, line 75, it should be 'preliminary evidence indicates' .</p> <p>Introduction: while it is stated that this is an exploratory analysis, the variables used in the analysis should still be justified. Why would they be relevant for the deterioration due to the pandemic?</p> <p>Method:</p> <ul style="list-style-type: none"> - the structure of the Method section should be improved along STROBE reporting guidelines. - it should be clarified that 'carer' refers to the informal carer (spouse, children, friends) not a professional carer (assuming this is the case) - why were there 3 functioning assessments used? While they do differ, there is also considerable overlap. A correlation analysis
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	<p>should be added to clarify that.</p> <p>Results: Supplemental table A should be scaled down and added to the main text as it summarizes the main outcome.</p> <p>Discussion: The first paragraph should be worded more carefully, to avoid overstating the results. Looking at figure 2, a substantial proportion of participants also improved, this should be considered in the Discussion. Also, why was FAST and PSMS related to psychosis subsyndrome but not IADL?</p> <p>Page 13 or 14, line 1: the study cannot confirm the impact of the pandemic using an observational design, causation cannot be inferred. All changes might have been part of the deterioration due to dementia. The discussion should also look at epidemiological studies pre-covid to compare changes within similar time frames.</p>
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REVIEWER	Hunt , Lauren UCSF
REVIEW RETURNED	22-Sep-2021

GENERAL COMMENTS	<p>I appreciate that the authors have responded thoroughly to the reviewers' concerns. I just have a few more suggestions that would help make this article clearer to readers.</p> <p>1) I find it confusing that table 1 has two columns for the same variables. I would make it a single column and then write the way the number is shown in the row, instead of at the top of the column. Or else the column header could be n(%) with an asterik indicating unless otherwise noted.</p> <p>2) For table 2, I appreciate that the authors switched the outcomes to median values, but now it's confusing how many zeros there are in this table. I think the issue is that there are a lot of people who did not experience any of the symptoms in the subcategories. Perhaps for the domain scores, you could report the n(%) of people with a score greater than 1?</p> <p>3) The paragraph in the discussion starting at line 283 could be strengthened. How can be better support PWD and their caregivers in the face of local and global events (e.g. infectious disease outbreaks) when visitation may be restricted, caregivers are facing additional stressors, etc?</p>
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REVIEWER	Kinosian, Bruce University of Pennsylvania
REVIEW RETURNED	18-Sep-2021

GENERAL COMMENTS	the authors have been responsive to comments, and have improved and clarified the presentation of their results.
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VERSION 2 – AUTHOR RESPONSE

Reviewer: 4

Dr. Bruce Kinosian, University of Pennsylvania
Comments to the Author:

The authors have been responsive to comments and have improved and clarified the presentation of their results.

Response: We thank you for acknowledging the alterations improving the manuscript.

Reviewer: 3

Dr. Lauren Hunt, UCSF

Comments to the Author:

I appreciate that the authors have responded thoroughly to the reviewers' concerns. I just have a few more suggestions that would help make this article clearer to readers.

1) I find it confusing that table 1 has two columns for the same variables. I would make it a single column and then write the way the number is shown in the row, instead of at the top of the column. Or else the column header could be n(%) with an asterisk indicating unless otherwise noted.

Response: Thank you for addressing readability. We have now made it a single column with n(%), mean(SD), and median [IQR] shown in the rows of table 1. This also applies to table 2 and supplementary table B.

2) For table 2, I appreciate that the authors switched the outcomes to median values, but now it's confusing how many zeros there are in this table. I think the issue is that there are a lot of people who did not experience any of the symptoms in the subcategories. Perhaps for the domain scores, you could report the n(%) of people with a score greater than 1?

Response: We appreciate this comment relevant to table 3. We now report n (%) of persons with dementia with a symptom load of clinical relevance using the established domain cut-off score of ≥ 4 (range: 0-12) which is a compound measure of frequency and severity. Additionally, we report the number of persons with a CSDD score indicating depression of clinical relevance. Recommendations regarding clinical cut-off scores are given and cited on lines 128 and 135-136 respectively and discussed on lines 255-257 in the manuscript.

3) The paragraph in the discussion starting at line 283 could be strengthened. How can we better support PWD and their caregivers in the face of local and global events (e.g. infectious disease outbreaks) when visitation may be restricted, caregivers are facing additional stressors, etc?

Response: We acknowledge that restrictions on visitation may be advantageous in the face of local and global events. We have now elaborated on the potential of digital communication to support dyads (lines 310-311). We suggest future research to investigate the effects of more moderate restrictions and service interventions (line 326). Additionally, we have now addressed your concern for informal caregivers in the final paragraph addressing unanswered questions and future research (line 329).

Reviewer: 1

Dr. Veronika van der Wardt, Philipps-Universität Marburg

Comments to the Author:

This is a well written paper with an interesting analysis of predictors of change in BPSD in people with dementia during the covid-19 restrictions in Norway. There are a few issues that should be addressed:

Abstract: please add mean time between assessments (and SD).

Response: We have accommodated your request on line 37.

There is a type on page 3, line 75, it should be 'preliminary evidence indicates'.

Response: Thank you for spotting this typo, which is now corrected (line 75).

Introduction: while it is stated that this is an exploratory analysis, the variables used in the analysis should still be justified. Why would they be relevant for the deterioration due to the pandemic?

Response: Thank you for addressing justification of variables used in the regression analysis. We have now described the procedure of selection of variables on lines 169-170, followed by procedure for model selection and testing.

Method:

- **the structure of the Method section should be improved along STROBE reporting guidelines.**

Response: We appreciate your comment and have restructured the method section according to STROBE. We provide the STROBE checklist according to revision.

- **it should be clarified that 'carer' refers to the informal carer (spouse, children, friends) not a professional carer (assuming this is the case)**

Response: Thank you for clarifying this to the reader. Throughout the manuscript, we have now replaced 'carer' with 'informal carer', accordingly.

- **why were there 3 functioning assessments used? While they do differ, there is also considerable overlap. A correlation analysis should be added to clarify that.**

Response: Thank you. We have now clarified the procedure of model selection (lines 169-170). Multicollinearity, robustness, and goodness-of-fit were checked for selected models. The functioning assessments were the covariates showing the strongest correlation (IADL-PSMS 0.72; PSMS-FAST 0.52; IADL-FAST 0.41), and we discussed whether to exclude some, but decided on keeping all three because they substantially improved the models evaluated by the Akaike Information Criterion. This is now stated on lines 173-174.

Results: Supplemental table A should be scaled down and added to the main text as it summarizes the main outcome.

Response: Accommodating your suggestion, we have reformatted and consequently downscaled the table and added it to the main text (table 4). The reader is also provided with the covariates showing associations in the main text (lines 218-231). We thank you for this important suggestion and agree that this information may be important to the reader.

Discussion:

- **The first paragraph should be worded more carefully, to avoid overstating the results. Looking at figure 2, a substantial proportion of participants also improved, this should be considered in the Discussion.**

Response: Thank you for nuancing the discussion; we have now rephrased the first paragraph (lines 244-246) and the wording throughout the manuscript to avoid overstating the results (lines 283, 284-291, and 317).

- **Also, why was FAST and PSMS related to psychosis subsyndrome but not IADL?**

With regards to factors associated to the psychosis subsyndrome, we do not know why we did not find a significant relationship of IADL although close to the margin of statistical significance ($p=0.08$) in our explanatory analysis. FAST and PSMS both assess daily functioning yet FAST is specifically developed as a dementia staging tool which could explain why we found this factor associated with worsening psychosis. Notably, there is considerable overlap between these assessments (lines 173-174) and due to the wide confidential intervals of covariates (lines 277-278) the results should be interpreted with caution. We address the need for complementary studies (lines 278-281, 325-329).

Page 13 or 14, line 1: the study cannot confirm the impact of the pandemic using an observational design, causation cannot be inferred. All changes might have been part of the deterioration due to dementia. The discussion should also look at epidemiological studies pre-covid to compare changes within similar time frames.

Response: Thank you. We have now modified the statement on line 280-281 according to your comment. We agree that randomization should be employed whenever possible but argue that use of nonrandomized data may be reasonable and necessary to evaluate causal effects in many cases, such as with the covid-19 restrictions (lines 284-286). The time frame of our study is discussed in relation to pre-covid studies on BPSD (lines 287-291).